

# HAMMOND (W<sup>m</sup>. A.)

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Subject of "Animal Extracts."

BY

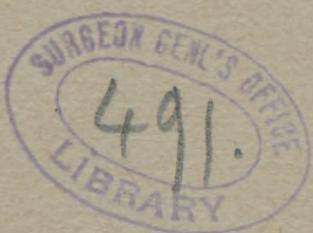
WILLIAM A. HAMMOND, M. D.,

SURGEON GENERAL, U. S. ARMY (RETIRED LIST).

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## A FURTHER CONTRIBUTION TO THE SUBJECT OF “ANIMAL EXTRACTS.”

BY WILLIAM A. HAMMOND, M. D.,  
SURGEON GENERAL, U. S. ARMY (RETIRED LIST).

FROM several communications that I have received from physicians who are engaged in extracting cerebrine according to my formula, I learned that they have taken special pains to get rid of the blood contained in the cerebral vessels, especially of that in the sinuses. This is all wrong, as to the intracranial blood is to be attributed, in my opinion, much of the physiological and therapeutical effect of the cerebrine. In the experiments performed several years ago by myself and others relative to the juice of the testicles, it was always found highly desirable not to remove the blood from the organs. In my published communications I have stated that the brain is to be carefully washed in a solution of boric acid, and this procedure is solely for the purpose of getting rid of all extraneous matter that might have come in contact with it after its removal from the skull and before it reached my laboratory. In the absence of any express direction to remove the blood, I took

it for granted that such a course would not be adopted. To this fact, and to the additional one of endeavoring to extract in a few days a substance that can not be obtained till after the lapse of several months—six at least, and twelve are better—must be ascribed the failure of a certain drug firm, who, without my knowledge or consent, chose to go into the business of manufacturing cerebrine for sale, to get anything but an entirely inert product—one, in fact, that can be taken in ounce doses without any other effect than would be obtained from a like quantity of a mixture of alcohol, glycerin, and a solution of boric acid.

It is well known that in old specimens of brain and other nervous tissue preserved in alcohol a substance crystallizes out and is deposited upon the surface of the specimens. This has received the name of “spermine.” It is only after several months and sometimes years that it makes its appearance. Chemical analyses now being conducted, but not completed, show that spermine is a constituent, though not the only one, of cerebrine and in greater or smaller proportion of the other animal extracts—medulline, testine, ovarine, cardine, and masculine—made by my processes. Undoubtedly its presence is due to the fact that the organs subjected to extraction contain a large amount of nerve tissue. Pure spermine,\* with which I have experimented to a large extent, acts in some respects, so far at least as the immediate effects are concerned, like cerebrine. It produces a similar vaso-motor disturbance, but not to as great a degree in corresponding doses. It certainly is not the only active principle of cerebrine or of the other animal extracts. I have not been

\* Manufactured by Messrs. Clinton E. Worden & Co., San Francisco, California.

able to ascertain that spermine produces any decided permanent effect upon the organism, though I have taken it myself in double and treble doses and given it to others to a like extent for several weeks at a time.

Whether, as I stated in my original communication, the essential factor of cerebrine is a substance pre-existing in the brain tissue and extracted by the menstruum or is a secondary product I do not know, nor, so far as its therapeutical properties are concerned, is it a matter of much importance. It is something which I know to be curative of certain morbid cerebral conditions, but whose thorough study is by no means yet accomplished.

The immediate effects of the cerebrine and of most of the other extracts are those indicating vaso-motor disturbance, and are so far similar to those produced by the nitrite of amyl and trinitrin or nitroglycerin. It is of much longer duration, but not so intense as that produced in the average person by a drop of a one-per-cent. solution of nitroglycerin. I have experimented largely upon myself and others with fresh extracts of the brain mixed with solutions of nitroglycerin of various strengths, and I am convinced of the great difference in the action of these mixtures and of cerebrine prepared according to my formula. It has been observed by those who have employed the extract of the thyreoid gland in the treatment of myxoëdema that even more severe symptoms usually follow the administration. These are in the form of giddiness, headache, faintness, and an apparent tendency to convulsions, and occasionally it has been found necessary to suspend the treatment on this account. Similar effects to those produced by cerebrine are generally observed from the hypodermic injection of the extract of nerve substance prepared by Dr. Gibier. I have never in a single instance—and I have used them many hundreds of times—observed

the slightest alarming effect from the use of the animal extracts prepared according to my method.

I have sometimes thought that the combination of glycerin, alcohol, and the highly nitrogenous substances from which these extracts are made might result in the production of a substance analogous to nitroglycerin, and in my first communication on the subject to the *New York Medical Journal* I threw out the suggestion of a secondary product being formed. As I have said, however, the thorough chemical analysis which is now being made will, I hope, dissipate the uncertainty which necessarily exists relative to the exact composition of these extracts.

A word as to one of the critics of my facts and theories. So far as the latter are concerned, I have not the slightest disposition to say a word or commit an act that may impede the fullest expression of opinion. But I think the discussion should be conducted in a decorous manner and that my sincerity of statement should not be impugned. I am therefore surprised to find that Dr. J. S. Leonhardt, who is evidently a physician with a scientific training, should, in the number of this journal for June 10th, adopt a sarcastic tone in regard to the theory and one of incredulity relative to the facts. I think my contributions to this subject have been written with moderation and frankness. I have deprecated extravagance, have held nothing back, and have claimed little, leaving it to the experience of the profession at large to determine the true value of the animal extracts that have formed the subjects of my studies. I am aware of all the difficulties connected with such an investigation; but having been one of the first, if not the very first, in this country to enter upon the practical study of experimental physiology and physiological chemistry, having held a professorship of those branches in a promi-

nent medical college, and having continued to work in them to the present time, I consider myself quite as competent to form an opinion on any point connected with those sciences as is Dr. Leonhardt.

In regard to the sphygmograms printed with my paper on Cardine I have only to say that they were made with Marey's sphygmograph, and are engraved from accurate pencil tracings made on the blackened paper of the apparatus. Every one who uses the sphygmograph will know how impossible it would be to send by mail from Washington to New York the original sphygmograms without the great risk of having them obliterated or seriously damaged before they reached the engraver. As regards their value, that is a matter of opinion, and Dr. Leonhardt has a perfect right to estimate them according to his own notions. Their authenticity, however, is quite another matter, and if he questions *that*, I have only to say that he commits a solecism against good manners for which it would be difficult to find an excuse. It is true, I can not boast of having “several thousand sphygmograms” in my possession, but I have been accustomed to the use of Marey's and Pond's sphygmographs, the one for over thirty and the latter for more than twenty years.

Dr. Leonhardt apparently ignores the fact that the tracings made by one kind of sphygmograph can not be compared with those made by a different instrument. Doubtless “Dudgeon's” sphygmograph—with which, however, I am not acquainted—is entirely competent to be used in the determination of the action of cardine upon the heart, but until Dr. Leonhardt ceases to theorize from the tracings of my sphygmograph to those of the one he is accustomed to he is not in a position to give a logical opinion on the subject.

Let him therefore experiment with “Dudgeon's”

sphygmograph as I have done with Marey's, and then, though doubtless the tracings will be different from mine, he will at any rate be competent to judge so far as his own instrument is concerned.

In point of fact, however, he admits that the sphygmograms printed in my paper teach to some extent exactly what I say they do—namely, that cardine increases the frequency and tension of the pulse, but he denies that they show increase of the force. I say they do, inasmuch as the up-stroke of No. 2 and No. 3 is less vertical—more oblique—than that of No. 1.

When Dr. Leonhardt, from the examination of tracings not made with an instrument with which he is working and with whose rate of movement he is unacquainted, undertakes, as he does in his criticism of tracing No. 2, to state the frequency of the pulse, he does that which every experienced sphygmographist knows can not be done with even an approach to accuracy. The length of the line between any two up-strokes depends on the velocity of the paper moved by the clockwork, and is rarely, if ever, the same in any two instruments of different makers or even in any two of the same kind. It is by this line that the frequency of the pulse is measured. When the paper moves rapidly the line is short, when it moves slowly the line is long. All that Dr. Leonhardt or any one else can say from a comparison of tracing No. 1 with tracing No. 2, so far as frequency is concerned, is that No. 2 indicates a more rapid pulse than does No. 1. The pulse making the tracing No. 1 was 68, of No. 2, 85, and of No. 3, 74, and I am able to state with absolute positiveness that the subject has no cardiac or arterial disease, notwithstanding Dr. Leonhardt's opinion to the contrary.

As to Dr. Leonhardt never having seen any similar

sphygmographic tracings, I can well believe his assertion ; but if he will kindly take the trouble to consult Marey's *Physiologie médicale de la circulation du sang*, Paris, 1863, he will find not "several thousand" perhaps, but enough, I think, to convince him that there is even yet something for him to learn on the subject.





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